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LEIBNIZ IN LONDON.¹

LEIBNIZ paid two visits to London from Paris, where he was staying from March, 1672, to October, 1676: the first visit, which was in connection with the embassy from the Elector of Mainz, was from January 11 to the beginning of March, 1673; the second was made on his way home to Germany, when he stopped in London for about a week in October, 1676.

Leibniz had a habit of writing out all the important scientific points in the correspondence that he kept up with noted people, so that he might thus impress them the more deeply upon his memory. I have discovered among his manuscripts three folio sheets on which he has written down the things worth noting in connection with these two visits to London.² The sheets which relate to his second visit have been known to me for some time; but the other ones, referring to the first visit, I came across only during my last stay in Hanover in the summer vacation of the year 1890.

In what follows, I have only paid attention to the contents of these sheets which refer to mathematics.³

¹ Translated by J. M. Child, from an article by Dr. Gerhardt in the *Sitzungsberichte der Königlich Preussischen Akademie der Wissenschaften zu Berlin*, 1891, pp. 157-165. The notes are by the translator.

² These highly important documents ought to be photographed and published in facsimile.

³ It seems a pity that Gerhardt has not given the contents of the section labeled "Mechanica," unless indeed this is all non-mathematical; there may

The sheet relating to Leibniz's first visit to London, of which I have added a partial transcript under the heading I, is divided on both pages into sections [the word used in the original is *Felder* = columns, but it will be seen that, according to the transcript given later, the sections are horizontal and not vertical], in which Leibniz has entered all that he considered to be worth noting. While the sections labeled "Chymica," "Mechanica," "Magnetica," "Botanica," "Anatomica," "Medica," and "Miscellanea" are filled up with an extraordinary number of memoranda, the first sections, which are allotted to mathematical subjects, are very poorly filled. That labeled "Geometrica" contains a note that is especially worth remarking: "Tangents to figures of all kinds. Development of geometrical figures by the motion of a point in a moving straight line."⁴ In all probability it may be supposed that this refers to the lectures of Barrow, delivered on his method of tangents at the University of Cambridge down to the year 1669. As is well known, the method of Barrow is only applicable to such curves as can be expressed by rational functions.⁵ Newton's name was mentioned in

be in it some intimation that would lead to a clue as to the origin of Leibniz's use of the word *moment*, meaning thereby, not Newton's use of the word, but the idea now familiar to us in the determination of the center of gravity of an area, expressed by the equation

$$x = \Sigma ax / \Sigma a,$$

where a is the element of the area distant x from the axis, x the distance of the center of gravity from that axis, and Σax is the sum of the 'first moments of the elements' or 'the first moment of the whole area.' See note 16, later.

⁴"*Tangentes omnium figurarum. Figurarum geometricarum explicatio per motum puncti in moto lati.*"

⁵In a footnote, Gerhardt asserts that "Barrow's *Lectiones Geometricae* appeared in 1672." This is incorrect; for they were published, combined with the second edition of the *Lectiones Opticae*, in 1670; nor can Gerhardt be referring to the second edition, for that appeared in 1674 and then as a separate volume. Also, I have, in the little book on *The Geometrical Lectures of Isaac Barrow*, published by the Open Court Publishing Co., given reasons for supposing that these lectures were never delivered as *Lucasian Lectures*, though they may have formed the subject-matter for college lectures at Gresham and Trinity. Again, it is not true, although "well known," that "the method of Barrow was only applicable to such curves as can be expressed by rational functions"; this remark is even only partially true about the differential triangle method; for, as I have shown in the above-mentioned book, Barrow had a complete calculus, which included, among other things, the important idea of *substitu-*

the "Optica." Leibniz has the remark: "They told me about a certain phenomenon that Barrow confessed he was unable to solve. Newton's difficulty has so far not been solved, Father Pardies having given it up."⁶ Obviously this remark applies to Newton's experiment on the refraction of light by a prism and to the decomposition of white sunlight, and especially to the fact that a circular solar image becomes after refraction a long spectrum. Father Pardies of Clermont had published in opposition to Newton his "Two Letters containing Animadversions upon I. Newton's Theory of Light," in the *Philosophical Transactions* of 1672, together with a letter from Newton.

It cannot be said for certain that Leibniz, during his first stay in London, met with any of the great English mathematicians; Wallis lived at Oxford, while Barrow and Newton resided at Cambridge.⁷ Indeed, it is made a matter of plaint by Brewster, the biographer of Newton, that the Royal Society of London at that time numbered few men of distinguished talents who were in a position to perceive the truth of the optical discoveries of Newton. In the letter which Leibniz addressed to Oldenburg, the Secretary of the Royal Society, during his visit to London, he men-

tion, which is all that is necessary to complete the "a-and-e" method and make it applicable to surds and fractions, and probably was thus applied by Barrow in working out his constructions; but the whole thing was geometrical, which apparently hid the inner meaning until recently.

To my mind, the mention of but "tangents and local motion" points out that, on Leibniz's *first* reading of Barrow, he only perused at all carefully the first five lectures, which are relatively unimportant; or rather it confirms an opinion I had already expressed to Mr. P. E. B. Jourdain.

⁶ "Locuti sunt mihi de phaenomeno quodam quod Barrovius fatetur se solvere non posse. Newtoni difficultas soluta hactenus non est, P. Pardies manus dante."

⁷ It seems however that Leibniz attended the meetings of the Royal Society; at any rate once, when he exhibited the model of his calculating machine. It would be interesting if the roll of members present on all occasions during this period could be obtained, as doubtless they were kept. For such men as Ward were members at the time and attended the meetings, and Ward was, if not in the same class as the three whose names are given, an excellent mathematician; and, Leibniz, being somewhat of a notable, on account of his connection with the Embassy from Mainz, would surely be introduced to all eminent members present.

tioned that he had met by accident the mathematician Pell at the house of Boyle, the chemist. The conversation fell upon those number-series which in elementary mathematics were called the higher arithmetical series and whose sums and terms were found by the help of differences. Leibniz showed that he had gone deeply into the study of such series and had partly found out some new methods for calculating the terms.⁸ Leibniz's letter to Oldenburg was dated Feb. 3, 1673 (1672 O. S.).⁹

From the preceding it appears that what Leibniz learned with reference to mathematics from his first visit to London was quite unimportant.¹⁰ The chief aim of his stay in London was to be elected as a Fellow of the Royal Society; and this came to pass, owing in part to an exhibition of a model of his calculating machine, and in part to the friendly offices of Oldenburg.

After his return to Paris at the beginning of March, 1673, Leibniz was able to find more leisure to follow up his studies without hindrance; the political mission which was the cause of his being sent to Paris, was now at an end.

It may be regarded as certain that, before his first visit to London, Leibniz made the personal acquaintance of the men with whom he corresponded before he came to Paris, and especially Antoine Arnauld and de Carcavy. The

⁸ The account given by Leibniz himself in the *Historia* (see *The Monist* for October, 1916) reads thus: "He" [for Leibniz wrote in the third person, under the guise of "a friend who knew all about the matter"] "also came across Pell accidentally, and described to him certain of his own observations on numbers, and the latter stated that they were not new, but it had been recently made known by Nikolaus Mercator. . . . This made Leibniz get the work of Nikolaus Mercator." As a matter of fact the suggested plagiarism, or what Leibniz took for such a suggestion, was from Mouton and not from Mercator. This is an instance of the lack of memory from which Leibniz suffered; such lack as caused him to make notes of all important points.

⁹ See Note 32, on the introduction of the Gregorian calendar.

¹⁰ I cannot see what reason Gerhardt has for this statement, considering the contents of Barrow's book, which we know that Leibniz had purchased; that is, unless we assume either that Leibniz, as I have suggested, did not at that time read the whole of Barrow, or failed to grasp what Barrow had given owing to his (Leibniz's) incomplete knowledge of geometry.

latter belonged to the circle in which Pascal moved. Whether at that time Leibniz had made the acquaintance of Huygens is not quite so certain; at any rate he did not come into close relations with him until after his return from London. Huygens presented him with a copy of his great work, *Horologium Oscillatorium*, which had just (1673) been published. The recognition that his mathematical knowledge at that time was insufficient to enable him to understand the contents of this book, combined with a reawakening of his former love for mathematics, had the effect of making Leibniz devote himself with the greatest fervor to the study of mathematical subjects. Cavalieri's method of indivisible magnitudes, the writings of Gregory St. Vincent, the letters of Pascal (which were especially recommended to him by Huygens), were used by him as guides in his studies. As the first-fruits of these studies, he obtained the theorem that, when the square on the diameter of a circle was taken as unity, the area of the circle was expressed by the infinite series

$$1 - \frac{1}{3} + \frac{1}{5} - \frac{1}{7} + \dots \dots \dots \text{ad inf.} \quad (11)$$

He obtained it thus: Instead of dividing the circle, as in the method of Cavalieri, into trapezia by means of parallels, he divided it into triangles by lines radiating from a point; the areas of these triangles being proportional to certain lines. With these lines as perpendicular ordinates a curve could be constructed that was divided by these ordinates into trapezia, each of which is double the corresponding triangle. In this way Leibniz obtained a curvilinear figure¹² whose area was double that of the circle, but which was expressed by a rational function, $x = y^2/(1 + y^2)$,¹³

¹¹ Leibniz's own date for the discovery of this result, usually alluded to by him as the "Arithmetical Tetragonism," is 1674; "But in the year 1674 (so much it is possible to state definitely) he came upon the well-known Arithmetical Tetragonism; . . ." (See *Historia*, in *The Monist*, Oct., 1916.

¹² See the first critical note, page 536.

¹³ See the first critical note, page 536.

of its coordinates; and, using a method that was similar to that employed by Mercator for the equilateral hyperbola, this area could be found (Quadratrix).¹⁴

For the rest of Leibniz's treatment, see the hitherto unpublished manuscript, given under II in the appendix that follows.

As was often the case in the first scientific studies of Leibniz, intimations of the great problems that occupied his attention his whole life through are found here in his first efforts in the domain of higher mathematics. First is it to be remarked that Leibniz abandoned the division of curvilinear figures into trapezia, as was done by Cavalieri, and instead divided them into triangles; from this he was led to the "characteristic triangle,"¹⁵ which formed the foundation in the application of the differential calculus. Further, Leibniz constructed, instead of the proposed curve, another of which the area could be found (the "quadratrix" as he called it); this method of procedure frequently occurs in the later works of Leibniz on the integral calculus. Closely connected also with this is the solution of the inverse method of tangents, that is, given the tangent, to find the curve.

In these first efforts of Leibniz in the domain of higher mathematics is clearly to be seen the influence of his study of the writings of Pascal.¹⁶ The French mathematicians Roberval and Pascal did not consider that Cavalieri's

¹⁴ Observe that Leibniz (or Gerhardt) employs this word in a different sense from that of Barrow, with whom it means the special curve whose equation is $y = (r - x) \tan \pi x / 2r$, a curve that is particularly connected with the circle.

¹⁵ This contradicts both Gerhardt and Leibniz himself, who said that he got it from a consideration of a figure used by Pascal in finding the content of the sphere. See also the critical note referred to in 12, 13 above.

¹⁶ I hope to consider this influence in a later number of *The Monist*, in connection with an essay by Gerhardt on this very point; when I shall endeavor to substantiate an opinion I have formed with regard to the earlier manuscripts of Leibniz, which were discovered by Gerhardt, and of which translations appear in *The Monist* (April, 1917). I suggest that these do not represent so much the record of his original investigations as notes made while using the works of his predecessors as text-books.

method was consistent with the rigorous requirements of mathematics;¹⁷ they reverted to the study of the Greek mathematicians, and especially to the writings of Archimedes, combining with their method the developments which Kepler, in particular, had brought about by the introduction of infinitely small magnitudes into geometry. Moreover, in connection with Pascal, it is to be observed that he generalized into a "barycentric calculus" the procedure used by Archimedes for the quadrature of the parabola by means of the equilibrium of the lever.¹⁸ This "calculus" enabled him to solve problems on the cycloid which his contemporaries had vainly attempted.¹⁹ It was not unknown to Leibniz that, since the time of Pappus of Alexandria, quadratures and cubatures had been calculated by the aid of the center of gravity (Guldin's rule, "Centrobarica"); certainly he was now led, by the works of Pascal, again to notice the methods for the determination of the center of gravity, and was also induced to attempt to extend and perfect them. The manuscript of

¹⁷ I fail to see how this statement can be completely reconciled with the following well-known quotation from the "*Lettre de A. Dettonville à Carcavy*" (1658):

"J'ay voulu faire cét avertissement pour monstrier que tout ce qui est démontré par les véritables regles des indivisibles se démontrera aussi à la rigueur et à la manière des anciens; et qu'ainsi l'une de ces Methodes ne differe de l'autre qu'en la manière de parler; ce qui ne peut blesser les personnes raisonnables quand on les a une fois averties de ce qu'on entend par là" (Vol. VIII, p. 352).

Pascal also says on p. 350: "... la doctrine des indivisibles, laquelle ne peut estre rejetée par ceux qui prétendent avoir rang entre les Geometres."

That is, the method of indivisibles does not differ from the method of exhaustions, except in the way the argument is put; and that the former must be accepted by any mathematician with pretensions to rank among geometers.

The page reference is to the edition of Pascal's Works in 14 volumes, in the series, *Les Grands Ecrivains de la France* (pub. Hachette et Cie., Paris, 1914).

¹⁸ Pascal calls it "la balance." It is worth noting in this connection that Pascal uses the word "*force*" and not "*moment*" for the product of one of his weights and its lever-arm; so that we must look elsewhere for the clue to the use of the word "moment" in this sense by Leibniz.

¹⁹ Several of the problems proposed were solved by Huygens, de Sluze, and Wren; but by special methods, which did not satisfy Pascal, who called for a general method. Later (1670) Barrow gives the rectification of the arc, as a special case of a general theorem (Lect. XII, App. 3, Ex. 2, see my *Barrow*, p. 177).

Leibniz which is dated October 25, October 26, October 29, November 1, 1675, and which contains the investigation on the center of gravity, is headed, "*Analysis Tetragonistica ex Centrobarycis.*"²⁰

It is worth remarking that in this Leibniz continues the method by which he had found the series for the area of the circle. Incidentally these studies were the first occasion for the introduction of the symbol for a sum, i. e., the integral sign (October 29, 1675); from this as the antithesis, the sign for the difference, i. e., the symbol for differentiation, resulted.²¹ The equation in which Leibniz first introduced the sign of integration was, in the notation of that time:

$$\frac{\text{omn. } l}{2} \boxed{^2} \quad \sqcap \quad \text{omn. omn. } \frac{l}{a}$$

that is,

$$\left(\frac{\text{omn. } l}{2}\right)^2 = \text{omn. omn. } \frac{l}{a};$$

for which Leibniz writes

$$\int \frac{l^2}{2} \quad \sqcap \quad \int \int \frac{l}{a}$$

that is, when $l = dy$,

$$\frac{y^2}{2} = \frac{1}{a} \int dy \int dy$$

After his return to Paris in March, 1673, Leibniz was in constant communication with Oldenburg, the Secretary of the Royal Society; the subjects being almost entirely mathematical. In this way he obtained his knowledge of the work of the English mathematicians. Oldenburg's mentor on all mathematical questions was John Collins, who possessed a very wide acquaintance among English

²⁰ A translation is given in *The Monist*, April, 1917.

²¹ See the second critical note, page 543.

mathematicians; and it was through him that what they had done was communicated. In this respect special mention is to be made of the letter from Oldenburg to Leibniz, dated July 26, 1676, in which Collins informed him of a collection of letters from English mathematicians that he had in his possession. Collins mentions in it particularly that script of Newton, of December 10, 1672, in which the latter makes a communication about his method for tangents to curves, which are given by an explicit algebraical equation; he remarks that the method is only a corollary to a general procedure for solving other problems, such as those relating to rectification, determination of centers of gravity and so on.²² Collins stated in addition that, besides what this letter showed, nothing further was known at that time about Newton's method. It was on account of these communications, and probably also on account of a letter from Newton to Oldenburg, of which Oldenburg sent a copy to Leibniz at Paris, that Leibniz was moved to make his return journey to Germany in October, 1676, by way of London. Leibniz stayed there about a week; he made the acquaintance of Collins, who willingly let him have access to his collection of treatises and letters.²³ What Leibniz found in them that he thought worth noting he set down

²² Leibniz, in the *Acta Eruditorum* for the year 1700, says, "I can affirm that, when in 1684 I published the elements of my Calculus, I did not know any thing more of Mr. Newton's inventions in this kind, than what he formerly signified to me by his letters, viz., that he could find tangents without taking away surds; . . ." As Newton says in the article in *Phil. Trans.*, Vol. XXIX, No. 342, Anno 1714 (usually called the "*Recensio*") this "is very extraordinary, and wants an explanation."

²³ This is feasible, but there is another alternative given by Dr. H. Sloman (*The Claim of Leibniz to the Invention of the Differential Calculus, English edition*, pub. Macmillan, 1860), which strikes me as even more probable. Sloman's points are as follows: (1) It is highly probable that Leibniz's week in London was the *last week of that month*. (2) Oldenburg had then in his possession two letters from Newton for Leibniz, dated Oct. 24 and 26; these he showed to Leibniz. (3) As Newton himself mentions, these were blotted and hastily written; and thus Leibniz asks, on this account, that Oldenburg should let him see the tract of Newton to which they refer; which tract Leibniz knew was in the possession of Oldenburg, that is, a copy of it. For the details of the argument, occupying ten quarto pages, see the above-mentioned book by Sloman, pp. 97-106.

on two folios; the one has the heading, "*Excerpta ex tractatu Newtoni de Analysis per aequationes numero terminorum infinitas.*" This is the paper which Newton sent in June, 1699, to Barrow, from whom Collins received it on July 30, 1699. Collins made a copy of it, and sent the original back; and the original was printed in the year 1711. The other sheet has the heading, "*Excerpta ex Commercio Epistolico inter Collinium at Gregorium.*" A partial transcript of both these sheets follows under the heading III.

With regard to the extracts from Newton's paper, it is to be remarked that Leibniz was interested in the treatment of algebraical expressions of powers and in the turning of irrational expressions into the form of series by means of division and root-extraction. He noted indeed many examples in their entirety. How to get to quadratures was known to him; he merely indicated the process by the sign of a sum, i. e., by the symbol of integration. On the other hand, the part on the numerical solution of affected equations was new to him, and this he copied out well-nigh word for word; this is the well-known Newtonian method of solution of equations by approximations. Leibniz passes over as well known to him the remark, made by Newton at the close of the quadratures, that the problems of rectification, determination of the content of solids, determination of the centers of gravity, can be solved in the same way, and also the general indication of the process to be followed in such cases. Then follows the solution of inverse problems, for instance, to find from the area the base, that is the axis of the curve. This Leibniz copied out word for word. In the same way Leibniz has extracted the conclusion of Newton's paper, "*Demonstratio resolutionis aequationum affectarum.*" At the end of his manuscript Leibniz adds: "I extracted this from the letter of

Newton, August 20, 1672, addressed to Newton."²⁴ Probably this means that from the letters referring to Newton, Leibniz picked out the letter dated August 20, 1672, addressed to Newton.²⁵ So far as the script can be deciphered,²⁶ its contents were a graphic representation of Newton's method of solution of equations by approximations by means of Gunter's scale. Gunter's line had been noted by Leibniz on his first visit to London.

Of quite special interest to Leibniz were the letters of mathematicians which Collins had collected; on a second folio he made excerpts from letters from James Gregory. In two letters from Gregory (1670) was Isaac Barrow extolled as the greatest, not only among living writers, but also among all those that had written before him (Barrow). Further Leibniz found among these letters the letter mentioned above of Newton to Collins of December 10, 1672;²⁷ he extracted what Newton had mentioned with regard to his method of finding the expression for the tangent to a curve. Leibniz added at the end of this extract, "This method differs from that of Hudde as well as from that of Sluse, in that irrationals need not be eliminated."²⁸

²⁴ The Latin, "*Excerpti ex Epist. Newtoni 20 Aug. 1672 ad Newton,*" as given by Gerhardt, seems somewhat unintelligible; especially the word *Newton*. What Collins had (or what Oldenburg, as suggested by Sloman, had) was a copy of a manuscript that Newton had sent to Barrow. Gerhardt says, "so far as the script can be deciphered"; perhaps the word *Newton* is an error of transcription, or maybe an error on the part of Leibniz, due to the juxtaposition of the *Newtoni* which comes just before. In any case, note 25 applies.

²⁵ I do not think Gerhardt's translation of the word *excerpti* is correct.

²⁶ Gerhardt does not state whether the extract is badly written (this would show that it had been done in a very great hurry, for Sloman says that Leibniz, in his matter for publication, wrote a beautiful hand), or whether spoilt by age; in the latter case, as old-time inks contained salts of iron, the manuscript might be restored by photography, by means of a special plate, that I understand is sometimes used for detecting forgeries in deeds and notes.

²⁷ The letter was sent to Barrow to be sent on to Collins, probably with the object of being communicated through the latter to others; Collins seems to have been the regular channel of communication at this period, in a similar way to Mersenne.

²⁸ So we find in a manuscript, dated July 11, 1677, first of all an allusion to Sluse's method of tangents, "in which the equation is purged of irrational or fractional quantities"; then the remark, "I have no doubt that the gentlemen

From these extracts it follows that the contents of Newton's letter were unknown to him at that time (Oct., 1676).²⁹

Regarding the verbal communications that Leibniz had from Collins during the second stay in London, Collins wrote to Newton from London on March 5, 1677 (1676 O. S.), that the representation of the roots of an equation by a series was discussed between them.

It is clear that Leibniz during his second stay in London had made himself more familiar with the results obtained by English mathematicians than he was before. The question now arises: What specially occupied his attention? What had particular influence upon his studies? It is seen that what Leibniz found in Collins's collection relating to algebraical analysis was new to him and excited his interest; also the verbal exchange of ideas between himself and Collins was upon the same subjects.

On the other hand, as regards the infinitesimal calculus, Leibniz obtained nothing during his second visit to London; he had made a progress, by the introduction of his algorithm into the higher analysis, beyond anything that came to his knowledge in London.³⁰ Also these algebraical results, at least for the next period, left behind no lasting impression; for among Leibniz's papers is to be found an extensive treatise, written on board the ship that carried him from London to Holland, wherein he considered the

I have just mentioned know the remedy that is necessary to apply"; then follows the rule for a quotient, and the remark that this will be sufficient for fractions; lastly the rule for powers, with the remark that this will be sufficient for irrationals. Later, he says, "This method has more advantage over all others that have been published than that of Slusius over all the rest, because it is one thing to give a simple abridgment of the calculation, and quite another thing to get rid of reductions and depressions."

Thus, after the sight of Newton's paper, his whole business has been to improve the method of Sluse.

²⁹ I read it quite otherwise; he has had information of some kind, whether from Oldenburg direct or from Tschirnhaus, while in Paris, and visits London with the express intent of seeing the original papers.

³⁰ See the third critical note, page 546.

fundamental principles of motion, in the form of a dialogue.³¹

It was in the letter to Oldenburg written from Amsterdam on November 18/28, 1676,³² which Collins spoke of in the letter to Newton mentioned above, that Leibniz first refers to the subject of the problem of tangents, and remarked that the method of Slusius was not yet very perfect.³³

KARL IMMANUEL GERHARDT.

CRITICAL NOTES ON GERHARDT'S ESSAY.

BY THE TRANSLATOR.

NOTE I. *The origin of Leibniz's "transmutation of figures."*

(Referred to in footnotes 12, 13, 15.)

In the manuscript, which follows under heading II, Leibniz appears to attach very considerable importance to the method of transmutation of figures, and to claim that he had originated it. This claim is not incontestible; indeed I am almost inclined to think it is a deliberate plagiarism to start with; but Leibniz has perceived

³¹ Could this possibly have had its rise in an effort on the part of Leibniz to understand fluxions, or rather the idea of fluxions as he had found it in Newton's paper?

³² In 1582, Gregory XIII had directed 10 days to be suppressed from the calendar, then in accordance with the Julian system of intercalation, in order to allow the error which had crept into the time of the vernal equinox, by which Easter-day was settled, to be put right. The Gregorian calendar was introduced into all Catholic countries the same year, in Scotland in 1600, in the protestant states of Germany in 1700, but not in England until 1752. At the same time the commencement of the legal year in England was altered from May 25 to January 1; thus we frequently find two years given for dates between January 1 and May 25; while there are two days of the month given for all months of the year. For instance, February 1673 in the new Gregorian calendar would be only February 1672 in the Julian, distinguished by the letters O. S. (Old Style); and this date was written February 167²/_s. Similarly the date November 1⁸/_{ss}, 1676, was the 28th of November in the New Style, and the 18th in the Old Style, the number of the year being the same, since the day did not lie between the 1st of January and the 25th of May.

³³ "*Methodus Tangentium a Slusio publicata nondum rei fastigium tenet.*" These are Leibniz's words; Gerhardt omits to translate the word *publicata*, which probably refers to the publication in the *Phil. Trans.* of 1672, by Slusius, of the rules of his method, illustrated by examples. Sluse had probably improved upon this before 1676, but there is no evidence on this point. It would seem as if the subsequent work by Leibniz, culminating in the manuscript of July 11, 1677, was largely an attempt to perfect the rule of Sluse as a rule, and that Leibniz, if ever, did not appreciate the idea fundamental in the calculus, namely that of rates, until very much later.

in it something which the original author did not. Can it by any chance be the case that, in conformity with several other instances of Leibniz's bad memory for details, he is confusing author and subject, when he speaks of "the great light that suddenly dawned on him, which the author had missed," the reference being to Pascal and the discovery of the differential triangle? Can it be that the true connection is that in considering the original work of the author of such transmutations of figures, he perceived the method for the arithmetical quadrature? For here he really has found a thing that the author missed though it was almost staring him in the face, his discovery being due to a habit that Leibniz had of writing down everything that he could get out of any particular figure or bit of work that he had in hand, whether it was relevant or irrelevant.

Wallis and Pascal had both hinted at the method, i. e., had used it in special cases, namely for proving the equivalence of the parabola and the spiral; and Leibniz was familiar with both these authors. Again, James Gregory had, in the words of Barrow (*Lect. Geom.*, Lect. XII, App. 3, foreword to Prob. IX), "set on foot a beautiful investigation about involute and evolute figures," i. e., polar and rectangular figures equal in area to one another. Of course, Leibniz may not have seen this work of Gregory until later; probably not, although in one of his manuscripts he gives a theorem of Gregory; this however does not count for much, for the very same theorem is given by Barrow (see my *Barrow*, p. 130) and we know that Leibniz had a Barrow in his possession. This book, judging by his words, "as in Barrow, *when his Lectures appeared*, in which I found the greater part of my theorems anticipated," Leibniz wishes to make his friends believe was the 1674 edition, and not the edition of 1670, which he bought on his first visit to London. Why did Leibniz wish to conceal this fact? I assert that the reason for doing so was the fear that seemed always to overshadow him, the fear of being accused of plagiarism, whether such was a true or a false charge. I am firmly convinced that Leibniz got his transmutation of figures from Barrow; to this conclusion I have only just come, it never having entered my head to look for it at the time that I wrote my articles for *The Monist* of October, 1916, April and July, 1917.

Before I bring forward my arguments, it is right to state as a preliminary that, just as in calculus nowadays we usually draw a curve with its convexity downward, and draw the tangent to meet the horizontal axis beneath the curve, so Barrow drew his curves with the concavity downward in many cases, mostly, I think, in order to fit the diagrams conveniently on the old-fashioned folding plates of diagrams, that in those days were added in batches at the end of a book (see a specimen I have given at the end of my *Barrow*); in other cases, he draws his figure on the left-hand side of the axis. Whichever figure he draws, he always did one thing, namely, he drew any supplementary figure he had need of *on the other side of his axis or base*. Leibniz almost invariably drew

his curve on the right-hand side of a vertical axis, and supplementary figures on *the same side*. Hence, in the extract from Barrow given below, I am to be excused for failing to notice before what is more than a mere similarity.

In the following extract from Barrow (Lect. XI, Prop. 24), I have added Barrow's proof, which I thought unnecessary to give in my book; the figures given are Barrow's own on the left, which has been "up-ended" on the right; the latter is to be compared with the several figures by Leibniz.

Barrow's Lectiones Geometricae, Lect. XI, Prob. 24.

If DOK is any curve, D a given point on it, and DK any chord; also if DZI is a curve such that when any point M is taken in the curve DOK , DM is joined, DS is drawn perpendicular to DM , MS is the tangent to the curve, DP is taken along DK equal to DM , and PZ is drawn perpendicular to DK , so that PZ is equal to DS ; in this case the space DZI is equal to twice the space $DKOD$.

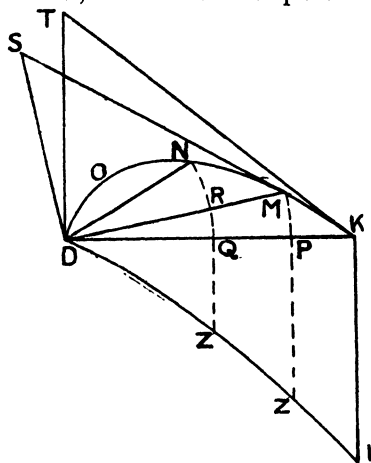


Fig. 1.

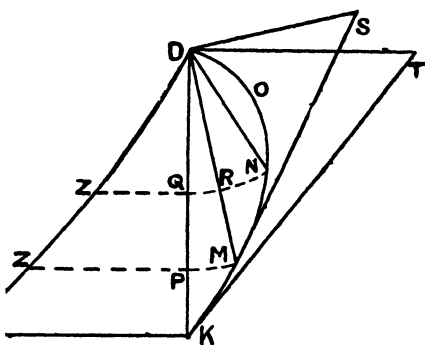


Fig. 2.

For let KP be considered to be indefinitely small, and let DT be perpendicular to DK and KT the tangent to the curve DOK . Then, drawing the arc MP , we have *as before*,

$KP:PM=KD:DT=KD:KI$, and hence $KP.KI=PM.KD$.

Take another small part PQ and, with center D , draw an arc QN through Q cutting the chord DM in R ; then *as before*,

$MR:RN=MD:DS$, $PQ:RN=MD:PZ$, $PQ.PZ=RN.MD$; and so on one after the other. Therefore, it is evident that the sum of all the rectangles $KP.KI$, $PQ.PZ$, etc., is equal to the aggregate of all the spaces $PM.KD$, $RN.MD$, etc.; that is, the space $DKI=2$ times the space $DKOD$.

The words I have italicized refer to Prop. 22, in which he uses

a similar though rather more complicated figure to reduce a polar area to a rectangle of *which one side is a given straight line*, and explains that the reasoning depends on the fact that the line DK is divided into infinitely small parts. Compare the words I have italicized with the description of Leibniz's method: "the areas of these triangles *being proportional to lines*."

Further, Barrow proceeds in Prop. 25 to prove the equivalence of the spaces formed (i) by applying each MS to the base and (ii) by applying each chord to the arc, previously rectified. And he winds up with the words: "Should any one explore and investigate this mine, he will find very many things of this kind. Let him do so who must, or if it pleases him."

This all suggests that Leibniz *did* explore this mine, that he did *not* invent the method of transmutation of figures for himself,

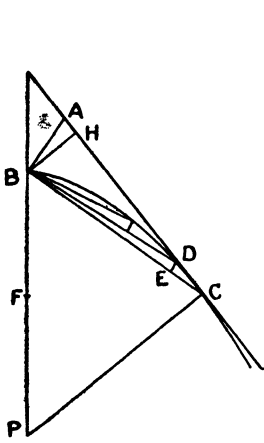


Fig. 3.

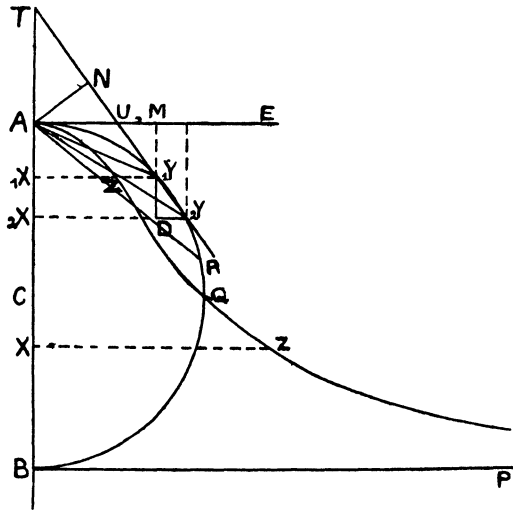


Fig. 4.

that he *did* find very many things of this kind, and that Barrow had *missed the arithmetical quadrature* construction; this Leibniz obtained through his regular practice of working every mine right out, to keep up Barrow's simile. Further comment is needless, I think, after a comparison of Barrow's figure (the up-ended version) with the figures of Leibniz given above.

Fig. 3 occurs in a manuscript November 21, 1675, which according to Leibniz is at least a year after he had discovered the arithmetical quadrature; and yet it has a heading, "A new kind of Trigonometry of indivisibles, etc." In this figure it is to be noticed that he has the perpendicular to the chord BC, agreeing with Barrow's DS and DT, but has not the tangent at the vertex that was necessary for the demonstration of the arithmetical quadrature. In the working in connection, he considers the similarity of all the tri-

angles possible, and notes as one point that "*the sum of all the triangles or the area of the figure is equal to the products of the AB's into the CE's*," which is Barrow's proof of Prop. 24 above.

Fig. 4 is the figure given in the *Historia* (see *Monist* for Oct., 1916), in connection with the explanation of how he found the area of the circle. Notice the difference between this figure and the one given in the manuscript that follows under the heading II, also that the description there given of the way in which he was led to it is much more natural. This is probably the true version, for the use of the notation B , (B) , $((B))$, points out that it was written at a comparatively early period, before Leibniz had adopted the prefix notation, ${}_1B$, ${}_2B$, ${}_3B$. In the account in the *Historia*, to which Fig. 4 applies, Leibniz says, "he once happened to have occasion to break up an area into triangles formed by a number of straight lines meeting at a point, and he perceived that something new could be readily obtained from it." I suggest that the occasion was most probably while he was digging in Barrow's mine! This is the reason why he has in the *Historia* given the figure more according to his usual practice, and different from the figure in the earlier manuscript, which is too much like a copy of Barrow's (query, where did Barrow get it from?). With regard to the figure and proof in the manuscript which follows, we find that the reasoning there given is unsound, unless Gerhardt has given us a slightly erroneous diagram; for Leibniz apparently does not perceive that the ordinates BA , which are equal to the corresponding CE , *must pass through the respective points D* , before he can say that one figure is double the other. Hence I conclude that at the date of this manuscript, the demonstration was imperfect and that he had no proof until he dug in Barrow's mine; in support of which conclusion I will quote from the *Recensio*, mentioned in footnote 22. "This quadrature, composed in the common manner, he began to communicate at Paris in the year 1675. The next year he was polishing the demonstration of it, to send it to Mr. Oldenburg, in recompense for Mr. Newton's Method, as he wrote to him May 12, 1676; and accordingly in his letter of August 27, 1676, he sent it, composed and polished in the common manner." This polishing, I take it, consisted in making the slight but important alterations in the demonstration and figure, from those given in the manuscript II that follows, to those given in the *Historia*.

What had he then got in July 1674, when he wrote to Oldenburg saying that he had got a wonderful Theorem, which gave the area of a circle, or any *sector of it exactly*, in a series of rational numbers? Or, when in the October following, October 26, 1674, he wrote to say that he had found the circumference of a circle in a series of very simple numbers; and also by the same "method" (a favorite expression of Leibniz) *any arc whose sine was given*? It was impossible that Leibniz could have had the two things that I have italicized; or at least, the latter was impossible to him, be-

cause the only way for him to obtain it *exactly*, i. e., to know the law of his series, was as yet unknown to him; unless we are to assume, contrary to his assertion, that the binomial theorem was known to him, which would involve his also having seen or been told about other parts of Newton's work. The only way open to Leibniz was to find the square root of $1-x^2$, and then its reciprocal by division; and this would not give him the law of the series, even if we assume that his knowledge of integration was sufficient to enable him to proceed any further. From his manuscripts it does not seem that even up to Nov. 1675 he had any further knowledge of integrations than that $\text{omn. } x = x^2/2$, and $\text{omn. } x^2 = x^3/3$; but as he says that he knows the latter from the quadrature of the parabola, there is some possibility that he might have been able to integrate every integral power of the variable from his reading of Wallis and Mercator.

However, there is the strongest probability that he had not got any proof for the two things italicized, and that the quadrature was in the same category. Where then had he obtained it? We find that in December, 1670, Gregory had found out for himself Newton's method of series; and two months later, February 15, 1671, sent several theorems to Collins, one of which was that now known as "Gregory's series." "And Mr. Collins was very free in communicating what he had received both from Mr. Newton and Mr. Gregory, as appears by his letters printed in the *Commercium*" (from the *Recensio*). One can imagine that Oldenburg would be one of the first to receive the information, and that for a certainty it would be passed on to Leibniz. I think then that Leibniz perceived that by putting $x=1$ in Gregory's series, and making the radius of the circle equal to unity, he could get an arithmetical quadrature; from that time onward he looked for a proof by pure geometry, and found it after reading Barrow's proposition referred to above; if we assume the possibility of integration of integral powers, it was an easy step to find that the series he had to integrate was $y^2/(1+y^2)$, and all he had to look for on his figure was a line of this length. This very well accords with the description of the way in which he found his demonstration, as given in the manuscript which follows under the heading II.

Lastly, in connection with the suggestion that I have made above, namely, that Leibniz had another method for his arithmetical quadrature than those he has given, there is one method that is bound up with the change that he made from the Pascalian characteristic triangle which he used at first, to the Barrovian differential triangle (see my note in *The Monist*, Oct., 1916, p. 615). In Example 5 of the method of the differential triangle (see my *Barrow*, p. 123), Barrow has found the subtangent for the curve $y=\tan x$, from a consideration of the figures on next page, and finds that

$$t = \frac{rr}{rr + mm} m. = \frac{CB^2}{CG^2} \cdot BG = \frac{CK^2}{CE^2} \cdot BG.$$

where r is the radius of the circle, m is the ordinate MP , which is equal to BG , and t is the subtangent TP .

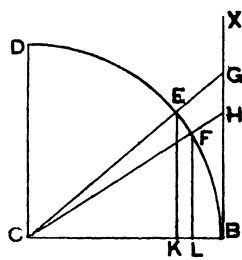


Fig. 5.

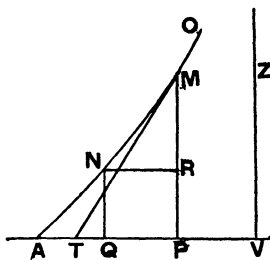


Fig. 6.

Now if we put the radius equal to unity, and for the ratio t/m substitute what was known by Leibniz to be equal to it, namely, QP/RM or EF/GH (by construction), we have the sum of all the EF 's is equal to the sum of ordinates equal to CK^2 (radius=1) applied to G at right angles to BG . Analytically, calling BG z , we have

$$\text{arc } BE = \text{sum. omn. } \frac{1}{1+z^2} \text{ applied to the line } z;$$

hence by division

$$\begin{aligned} \text{arc } BE &= \text{sum. omn. } (1 - z^2 + z^4 - z^6 + \text{etc.}) \\ &= z - z^3/3 + \text{etc.} \end{aligned}$$

I can hardly see how Leibniz could have missed this, with his analytical mind, even although Barrow has missed it; but there is a strong probability that at the time of writing, Barrow had not seen the quadrature of the hyperbola by Mercator, and, if he had, such algebraical work would not have appealed to him at all.

As far as I can make out, there is only one other alternative, which involves a direct contradiction of Leibniz's own statement; that is that his proof was not by the transmutation of figures in the first instance. Color is lent to this view by a letter of Leibniz and other papers, quoted by Sloman (pp. 131ff, in the English edition of the work referred to in footnote 23); also even by a passage in the *Historia* (see *Monist*, Oct., 1916, p. 599), where, while giving the story of the discovery of the arithmetical tetragonism, Leibniz distinctly hints at an algebraical method; for he says immediately afterwards, "The author obtained the same result by the method of transmutations, of which he sent an account to England." This reads as if he had another method in addition to the method by transmutations.

Let us consider this algebraical method. To square the circle, Leibniz has to integrate $\sqrt{1-x^2}=y$, say; let $y=1-xz$, then $y=(1-z^2)/(1+z^2)$, which is rational; moreover, he would also have been able to have substantiated his statement that at this time he

also had a proof of the series for the arc whose sine was given, for which he would only have had to integrate $1/\sqrt{1-x^2}$. But one cannot conceive that Leibniz had any means of expressing the element of z in terms of the element of x . Geometrically, he was incapable of it, without using Barrow's infinitesimal method; and of this we find the first instance in a manuscript dated November 1, 1675. Algebraically, he could not, for at this same date he could not differentiate a product. How then are we to account for the fact that he says he has a method for demonstrating both series for the arc, given the sine or the tangent? I think I can answer this. Many times we find assertions made, not only by Leibniz in those times, but by others in other times, of the possession of discoveries, when all that the assertor has is the idea of how they may be obtained. Thus, in the passage quoted, the concluding statement is, "and thus again all that remains to be done is the summation of rationals." So that if we accept this alternative we are bound to come to the conclusion that Leibniz did not yet recognize, what he ought to have done from the work of Pascal, that an area was not a mere summation of lines, but of rectangles formed by these lines ordinated at certain definite points along a straight line. That is to say, he did not recognize the fundamental principle of integration, namely, the importance of the factor dx or dz . When he had to write out his proof he found that the summation of $(1-z^2)/(1+z^2)$ or its reciprocal was beyond him; or rather that the series he found by Mercator's method was not correct; he had to resort to the geometrical proof, of which he got the idea by digging in Barrow's mine, as above; he found that this would not work for the other series; and consequently he dropped all claim to the second series. In his letters of 1676, therefore, we find him offering to send Newton the proof of his quadrature in return for the method of proof of the series for the arc when the sine is given.

Thus I come to the conclusion that Leibniz obtained these series in some way by correspondence, thought he had got a proof of his own, (which turned out to be incorrect), and much later did obtain a proof of his arithmetical quadrature by the transmutation of figures, *after obtaining the idea from Barrow*. As the special case, when x = the radius, had not been specifically mentioned by Gregory, Leibniz considered that he had a right to claim it, more particularly as he thought he had devised a proof for it, if it was necessary to produce one; for of course, Gregory had given no proof according to the usual custom of the time. Then, when he did find a proof, after having found that his original idea was hopeless, one can hardly blame him for sticking to his claim.

NOTE 2. *On the introduction of the Leibnizian algorithm.*

(Referred to in footnote 21.)

The two passages in which the signs for integration and dif-

ferentiation are respectively introduced occur in the manuscript of October 26, 1675.

i. "It will be useful to write f for omn., so that $f l = \text{omn. } l$, or the sum of the l 's."

ii. Not for some time is the sign for differentiation introduced, and then in these words: "I propose to return to former considerations. Given l and its relation to x , to find $f l$. Now this comes from the contrary calculus, that is to say if $f l = ya$. Let us assume that $l = ya/d$, or as f increases, so d will diminish the dimensions. But f means a sum, and d a difference. From the given y , we can always find ya/d or l , or the difference of the y 's. Hence one equation may be changed into the other,...."

Now of these the introduction of the symbol for integration can no more be called an invention than the use of Σ to stand for "the sum of all such terms as." It was simply, as Leibniz himself says, a convenient and useful abbreviation for *sum.omn.* or *omn.* It is nothing more or less than the long s then in general use; indeed it was so thought of by contemporary mathematicians, Newton for one at any rate, for we find in the *Recensio* the passage, "Mr. Leibniz has used the symbols sx , sy , sz for the sums of ordinates ever since the year 1686." This may have been an instance of prejudice, or perhaps the printers of the *Phil. Trans.* may not have had an integral sign in their fonts of type; but it shows up the fact that the English accepted it as the initial letter of the word "summa."

Now let us consider the introduction of the letter d . Gerhardt says that it resulted as antithesis to the sign f . How he can possibly derive this from the context I cannot surmise. I am well aware that in another passage he was unable to assign a meaning to the introduction of a letter, which was, to me, clearly used for the simple purpose of keeping the dimensions correct. We have this use again in the present passage. Leibniz knows that the sum of the lengths, $f l$, is an area; hence taking y to represent a length, given in terms of x , he introduces the *length* denoted by a to give with y the area of a rectangle. Therefore he argues that l must be an area divided by a length, and he writes $l = ya/d$, where d is *another length, introduced to keep the dimensions correct*. This is clear from the sentence that follows next: "so will d diminish the dimensions."

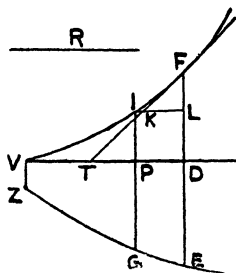
So far the sequence of ideas is easy to follow, and there is not the slightest trace of any concept of differentiation, nor, if the l 's are ordinated to any axis, any trace of a connection between d and an element of that axis. The difficulty begins with the next sentence: "But f means a sum, and d a difference." The first idea that strikes one is that this was added later, after that he had found out the connection between the inverse-tangent problem and quadratures. Gerhardt gives no suggestion on the point, so until the paper can be reexamined for small details like differences in the ink or character of the writing this idea will be disregarded. The next is that about this time he was reading Barrow, and then one is at once reminded

of Lect. X, Prop. 11 ; this is the proposition in which Barrow proves that differentiation is the inverse of integration. If we consider this in the manner of Leibniz, we get the equivalent that is set down on the right-hand side below :

BARROW

Let ZGE be any curve of which the axis is VD; and let ordinates applied to this axis, VZ, PG, DE, continually increase from the initial ordinate VZ; also let VIF be a line such that if any straight line EDF is drawn perpendicular to VD, cutting the curves in the points E, F, and VD in D, the rectangle contained by DF and a given length R is equal to the intercepted space VDEZ; also let $DE:DF=R:DT$, and join DT. Then TF will touch the curve VIF.

Cor. It should be observed that $DE \cdot DT = R \cdot DF = \text{area } VDEZ$.



LEIBNIZ

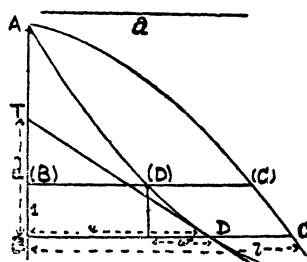
Let AC be a curve, whose axis is AB, and let the ordinate AB be l :

let AD be another curve, having the same axis, and let its ordinate DB be called y ;

let this curve AD be such that the area ABC, i. e., all the l 's or ll , is equal to the product of BD and a fixed line, i. e., equal to ay ;

then, taking $B(B)$ equal to unity, we have $l = aw$, where $w: B(B) = DB:BT$, or $w = y/d$, i. e.,

$$l = ay/d.$$



We thus see that the d that results as the “antithesis to the integral sign” (*als Gegensatz....sich ergab*), is not a difference at all, but the subtenant; it is y/d or w (on account of $B(B)$ being taken as unity) that is the difference between the ordinates y . But there is not the slightest trace of the idea of differentiation; this is made more manifest by the work which follows, which is based on his idea of obtaining independent equations, and eliminating all variables but one and thus reducing the problem to a quadrature. And yet he seems to perceive from the equation that gives the difference of the y 's as a *quotient*, that in some unintelligible way a division means a difference. Later therefore we find him trying to find an interpretation of d as an operator, whether he writes it

in front of his y , or as a denominator; namely, when he considers what value he is to assign to $d(xy)$. I venture to assert, unless we assume that Leibniz is considering this proposition of Barrow's, that there is no possible connection to be made out between the several sentences of this passage. Also that in no sense can this introduction of the letter d be looked on as an algorithm with any idea in it of differentiation.

I am well aware that in the above I have adduced no positive proof that my idea is correct; I have not had the advantage of Gerhardt in seeing these manuscripts. But I have honestly tried to find other ways of explaining the circumstances that lead from y/d as a *quotient* to dy as a *difference*, and I can find none other that is feasible than that given above, namely, that, perhaps by accident, Leibniz uses d for the subtangent (instead of the usual t), and perceives from such a figure as the above (which of course I do not intend to say he has given) that y/d (where d is the subtangent) works out the same as dy (when dx is taken to be unity); in other words the subtangent d is equal to $y/(dy/dx)$.

NOTE 3. *On the progress made by Leibniz before November, 1676.*

(Referred to in footnote 30.)

The remark made by Gerhardt that Leibniz "had made a progress, by the introduction of his algorithm into the higher analysis, beyond anything that came to his knowledge in London," is, to say the least of it, a matter of opinion. From a study of the six manuscripts, that Gerhardt has given us, that bear dates between that of the introduction of the integral and differential symbols (Oct. 26, 1675) and that of his return to Germany, via Amsterdam (after Nov., 1676), I fail to see that there is very much occasion for the main part of the above statement, namely, that the progress made by Leibniz was at all greater than anything that came to his knowledge in London; as for this progress, if for a moment we assume its superiority, being due to the reason set in italics, I fail to see that Gerhardt has any grounds whatever for such a statement.

The six manuscripts in question have been given, translated into English and annotated in *The Monist*, April, 1917; but for convenience I here add a precis of them.

- i. Nov. 1, 1675. A continuation of the work on moments about axes; the new symbols do not occur, *omn.* being still used. He has now read Wallis, Gregory and Barrow, in addition to Cavalieri and St. Vincent; he speaks of his theorem of breaking up a figure into triangles as bringing out something new; the whole tone of this manuscript is in the main Pascalian.
- ii. Nov. 11, 1675. He successfully obtains a solution of the problem of finding a curve such that the rectangle contained by

the subnormal and ordinate is constant. This he considers to be "one of the most difficult things in the whole of geometry." He uses the integral sign, and the denominator d ; but neither integration nor differentiation, the fact that $y^2/2d=y$, being taken from the "*quadrature of the triangle*." In verifying his result he quotes Slusius's Rule of Tangents. Further on, he has the note that x/d and dx are the same thing, though there is nothing to show why he comes to this conclusion; see the last critical note. He also comes to the conclusion that $d(xy)$ is not the same as $dx \cdot dy$; but in the last bit of work in this manuscript he uses special letters for the infinitesimals, showing that he has been trying to find the effect of d as an *operator*, or perhaps trying to find the reason of the equality x/d and dx . He has failed to solve a problem, which results in the differential equation, as we should now write it, $x+y \cdot dy/dx=a^2/y$, or as Leibniz has it $x+w=a^2/y$; although he gives an incorrect solution, which he asserts to be true. This time he does not attempt to verify his solution, the reason being obviously that he is unable to do so, because one side of his equation is a product. As a matter of fact, I have it on the authority of Professor Forsyth that there is no solution of this equation in elementary functions; or at least he says that he has been unable to find one, which I take it comes to the same thing. The one advance that can be found here is the appreciation that squares and products of infinitesimals can be neglected, as he has doubtless found in reading Barrow. It is worth noting that he now uses the differential triangle in Barrow's form instead of the form he says he got from Pascal.

- iii. Nov. 21, 1675. In this manuscript he sets himself another problem, which he fails to solve; the curve required is logarithmic, and this fact even he fails to bring out. In generalizations that arise from the consideration of his problem he obtains $dx \cdot y = xy - x \cdot dy$, in a more or less analytical manner; but immediately afterward states that nothing new can be obtained from it; he has already obtained this formula by his consideration of moments, geometrically; and he does not appreciate the advance there is in obtaining it algebraically. The manuscript concludes with a consideration of the figure by means of which it is generally supposed that he affected his arithmetical quadrature. This is very remarkable on account of the heading, which reads, "*A new kind of Trigonometry of indivisibles, by the help of ordinates that are not parallel but converge.*" What I refer to is the use of the word *new*, which I have here italicized. It is to be observed that the diagram and the results are almost identical with those of *Barrow*, Lect. XI, Prop. 22-24 (see the first critical note). He concludes by a reference to the trochoids,

- which shows that he is still under the influence of Pascal, if indeed he is not still studying his works.
- iv. Nov. 22, 1675. He returns to the subjects of the previous day. But there is here no mention of the signs of integration or differentiation.
 - v. June 28, 1676. Here we have a certain advance, for there occurs the statement: "The true general method of tangents is by means of differences." While he uses dy and dz for the elements of y and z , he uses β for the element of x ; the rest of the work is merely Barrovian in principle. This mere substitution of dy and dz for the special letters used by Barrow for the same things can hardly be called progress. What progress there might be is barred by the use of equations with three or more variables in them.
 - vi. July, 1676. The remark on the last manuscript is corroborated by the contents of this manuscript. Leibniz asserts that he has solved two problems, of which Descartes had alone solved one, and owned that he could not solve the other. The truth is that he has not solved the former, which was fairly easy, only given an alternative construction which is, if anything, more difficult to carry out than a construction from the original data for the curve. The latter he gets out in a hazy fashion ("...which belongs to a logarithmic curve"). This conclusion he comes to after several erroneous steps of reasoning; whereas the solution stared him in the face about a quarter of the way through the work, where he has the equation $c dy = y dx$, if he could have integrated dy/y with certainty. The failure I think arises from the study of Pascal, who lays it down that only one of the variables can increase arithmetically, and Mercator's work has been with y increasing arithmetically, and Leibniz has already considered that the x is increasing arithmetically (See my notes on this manuscript in *The Monist* for July, 1917).

Throughout the whole of these manuscripts, he makes no progress, because he is hampered by the idea of keeping one of his variables increasing uniformly; he seldom uses his algorithm for differentiation; and when he does do so, it is merely a substitution of dx , etc. for the special letters used by Barrow. In fact these manuscripts appear to me to be the records of his work on the textbooks of his study, Pascal, Wallis, Gregory, and Barrow; and we see him trying to fit the matter and methods found in them into his own ideas and notations. It is not until November, 1676, when he has arrived on the Continent, after having seen Newton's paper, that we have any Differential Calculus; even then some of the standard forms that he gives are not quite correct; on the other hand, he gives the method of substitution to differentiate an irrational, though he uses the Barrovian method to differentiate the general equation of the second degree, merely using dy and dx instead of

Barrow's special letters. It is not until July, 1677, that he is able to give anything like an intelligible account of the differentiation of products, powers, quotients and roots. Lastly I doubt if Leibniz ever did really appreciate the Newtonian idea that dy/dx was a *rate*, or else the example he gives of the use of the second and third differentials in his answer to Nieuwentiit would not have contained so many ridiculous errors.

TRANSLATIONS OF THE MANUSCRIPTS

Alluded to by Dr. Gerhardt.

I.

Scientific memoranda of the visit to England at the beginning of the year 1673.

When at the beginning of the year 1673, I accompanied his Excellency the Ambassador of Mainz, Baron Schörrnborn, a nephew (on his father's side) of the Elector, from Paris to London, although I stayed in England scarcely a month, among various distractions, I still gave attention to increasing my knowledge of philosophy; for at that time the English held a high reputation in this subject.

To set out a long minute record of daily happenings is useless on account of its inequality; for the fortune of all the days was not the same; indeed the points worth remarking heaped themselves up one day, and the next gaped with emptiness. For this reason perhaps it will be more satisfactory to go by heading of subjects, one remark recalling another as it were.

The principal heads for the subjects noted may be taken as Arithmetic, Geometry, Music, Optics, Astronomy, Mechanics, Botany, Anatomy, Chemistry, Medicine, and Miscellaneous.

ARITHMETIC. The line of proportions or Gunter's lines or the double scale. Logarithmotechnia or compendium for calculating logarithms. To recognize square numbers from non-squares by their end figures. Morland's machine.

ALGEBRA. Substance of English algebraical work of 27 years. Algebra of Pell. At first few rules, but lots of selected examples. Renaldinus not thought much of in England.

GEOMETRY. Tangents to all curves. Development of geometrical figures by the motion of a point in a moving line.

MUSIC. Its universal character. System of Birthincha. Vossius will publish Music.

OPTICS. They told me of a certain phenomenon that Barrow confessed that he was unable to solve. The difficulty of Newton hitherto unsolved, Father Pardies giving it up. Hook adheres to a catadioptric instrument of 9 feet, because for another of 50 feet movement inconveniences them. The secret of the largest aperture which can be given to microscopes is primarily as great as the distance of the object.

ASTRONOMY. Arrangement of Hook for observing whether the earth at any time sensibly approaches or recedes from the fixed stars, from which it can be judged that it is not in the center of the universe; he erected it in a fine tube set perpendicularly, and observed the stars that are vertically overhead. He, lying flat on his back, observed their dimensions most exactly.

CHEMISTRY.

MECHANICS.

PNEUMATICS.

METEOROLOGY.

HYDROSTATICS.

NAVIGATION.

MAGNETISM.

PHYSICS.

BOTANY.

ANATOMY.

MEDICINE.

MISCELLANEOUS.

II.

[This manuscript is very lengthy, the translation running to

about 6000 words, of which the first 5000 are written as a concise history of all the great geometers and their works, that are antecedent to Leibniz himself. This part is quite unimportant for the purpose of estimating the part that was played by Leibniz, and it passes my comprehension why Gerhardt should give it at length, while he has condensed the other two, which are really important. Hence, in what follows, I have given a precis of the first 5000 words, with here and there quotations, in which Leibniz has something to say that is either critical of the work of others, or a claim to superior knowledge or better method of his own. The last part, which purports to be the history of his arithmetical quadrature, together with his claim to the surpassing value of his achievement, I have given in full.]

(Precis). Geometry is a modern thing, probably due to the Greeks. The great name among the Ancients is that of Archimedes, who first used indivisibles; this use was more profound than that of Cavalieri, but the method became lost. The name of Apollonius must not be altogether omitted.

The learning of the Greeks passed on to the Arabs, who conquered them; among these we have Alhazen, and a certain Mahomet, who gave the formula for the general quadratic.

This brings us to the cubic and biquadratic equations, which were solved in the sixteenth century. The cubic is due to one Scipio Ferreus of Bologna; one of his pupils set the solution as a challenge after Scipio's death; Tartalea took up the challenge, found a solution and told his friend Cardan; the latter extended it and published it without the consent of Tartalea. Vieta, Descartes, and Ferrarius gave the solution of the biquadratic. But even Descartes and Vieta failed at equations of higher degrees. With regard to the work of Descartes, Leibniz remarks that "its origin [that is, of the method of solution] was a widely different and more fertile spring; and if Descartes had only recognized this, he would have rendered the discovery of Scipio more general and carried it to further heights. *But what has befallen me in this connection I will say in another place.*" Leibniz further remarks that the method of Descartes fails to give the roots of equations of higher degree, although the quality of the roots may be learned through it. "I will show in another place that the *reason for this is clearly known to me from the most fundamental principles of the art, and that I have established an extremely easy method, and one that is adapted too for enlarging science, by the many things that follow from it.*"

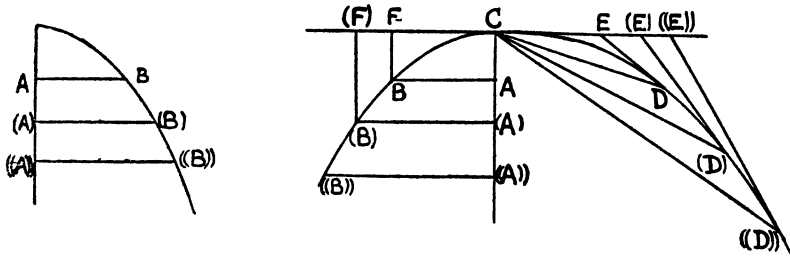
In the seventeenth century, Leibniz goes on to say, after Archi-

medes and Galileo's several times and influence are gone by, there is no writer from whom more is to be learned than from Descartes; and yet he is "*unable to pass over certain boastful remarks that he makes, by which the less experienced among us may be led into error.*" Descartes had said that by his method every geometrical problem could be reduced to the finding of the roots of equations. Leibniz remarks that this shows Descartes's ignorance of the matter. "For when the magnitude of curved lines or the space enclosed by such is required (which happen more frequently than perhaps Descartes thought, since he had not applied himself sufficiently to the 'mechanics' of Galileo), neither equations nor Cartesian curves can help us, and *there is need of equations of a totally new kind, of constructions and new curves, and finally of a new calculus, given so far by nobody, of which, if nothing else, I can now give certain examples at least, which are remarkable enough.*" . . . *I have mentioned these things so that men may understand that there are certain methods in Geometry, for which they may look in vain in the works of Descartes.*"

Returning to geometry purely, Leibniz next mentions the work of Galileo, Cavalieri (whose method he considers is rough and limited in extent), Torricelli, Roberval, Pascal, Wallis, Huygens, and Slusius, as contributors to the new geometry. He considers that a new epoch opens with the work of Neil and van Huraet (on rectification of curves), James Gregory, and Brouncker. "Finally Mercator gave a general formula for the area under a hyperbola." He claims Mercator as "an eminent German geometer"; but rather decries his discovery as being an easy one, on account of the ordinates working out as rational in terms of the abscissa. "But it was not so easy to give the magnitude of the circle, and its parts, expressed as an infinite series of rational numbers; . . . for the circle, however you treat it, has ordinates that are irrational. However I, as soon as I had found a certain very general theorem, by means of which any figure whatever could be converted into another that is quite different from it, but yet of equivalent area, set to work to try whether the circle could not be converted in some way into a rational figure; and the thing came out beautifully; . . . it will be worth while here to give a short account of the matter."

(In full). Nearly everybody who has up to now treated of the geometry of indivisibles has been accustomed to break up their figures into rectangles or parallelograms only by means of ordinates

parallel to one another. But the reasoning of Desargues and Pascal always pleased me very much; these in Conics, as we can call them in general, include under the name of ordinates not only parallels, but also straight lines meeting in or converging to a point, especially when parallels are included under the name of converging, by saying that the point of convergence goes off to an infinite distance. Thus while others only consider parallel ordinates, and have broken up their figures into parallelograms $AB(B)(A)$, $(A)(B)((B))$ $((A))$, in the way that Cavalieri does, I employ converging lines and resolve the given figure into triangles $CD(D)$, $C(D)((D))$, and at once draw another figure of which the ordinates AB , $(A)(B)$, etc., are proportional to these triangles.



Now this is the case if the AB 's are equal to the CE 's where it is supposed that the straight lines DE are tangents to the given curve; for in that case, as I will show below, it will come out that the space $B(B)(A)A$ will be double of the segment $C(D)DC$, and for any figure such as $C(D)DC$ another that is equivalent to it can be drawn. Now, supposing that the curve $D(D)((D))$ is circular and that CA is a part of the diameter, then, calling CA or FB x , and CF or AB y , and the radius of the circle unity, calculation will show that the value of x is $2y^2/(1+y^2)$. Thus the ordinate FB or x can be expressed rationally in terms of the given abscissa CF or y . Such figures as these, in which the ordinates can be expressed rationally in terms of the abscissae, I call rational. Thus we have drawn a rational figure equivalent to the circle, and this will be soon seen to be sufficient to give the arithmetical quadrature of the latter. For, from the sum of a geometric series of an infinite number of decreasing terms that is well known to all geometers, it follows that $y^2 - y^4 + y^6 - y^8 + y^{10} - y^{12} + \text{etc.}$ to infinity is the same as $y^2/(1+y^2)$, i. e., the same as $\frac{1}{2}x$, if only we understand that y is a quantity that is less than the radius, or unity. Now, since we have to collect together the infinite number of $\frac{1}{2}x$'s into one sum, in order

to obtain the quadrature of half the figure $C(F)(B)BC$ and what it comes to, namely, that of the circle; so also have we to collect together the infinite number of series $y^2 - y^4 + y^6 - y^8 + y^{10} - y^{12} + \text{etc.}$, into one sum, and this by the method of indivisibles and infinites can be done without difficulty. For, suppose that the last y , which in general is taken as $C(F)$, to be b , then the sum of every y^2 will be $b^3/3$, and of every y^4 will be $b^5/5$, and of every y^6 will be $b^7/7$, and so on; hence, the sum of the infinite number of $\frac{1}{2}x$'s, or of the series $y^2 - y^4 + y^6 - y^8 + y^{10} - y^{12} + \text{etc.}$, i. e., the area of half the space $C(F)(B)BC$, will be $b^3/3 - b^5/5 + b^7/7 - b^9/9 + \text{etc.}$ From which, by the help of ordinary geometry, it can be easily deduced that the square on the diameter is to the area of the circle as 1 is to $1/1 - 1/3 + 1/5 - 1/7 + \text{etc.}$; also speaking in general, supposing b to be the tangent, then the arc is $b/1 - b^3/3 + b^5/5 - b^7/7 + b^9/9 - b^{11}/11 + \text{etc.}$ Hence it now follows that any one without the help of tables and continual bisections of angles and extractions of roots can approximate to the magnitude of the arc to any degree of accuracy desired, so long as the tangent b is a little less than the radius; so that if we take the tangent to be a little less than the tenth part of the radius, the arc may be obtained with sufficient accuracy. Let us take the tangent to be a tenth part of the radius, then if we want the arc, it will be

$$\frac{1}{10} - \frac{1}{3000} + \frac{1}{500000} - \frac{1}{70000000} + \frac{1}{9000000000} = \text{etc.};$$

and reducing all to a common denominator, and adding the numbers into one sum (for it is not worth while going any further), then the arc will be a little greater than $518027821302775/519750000000000$, and the defect of this value from the true value will be less than the $1/1000000000000$ part of the radius. For if we do not subtract the last term, $1/1100000000000$, the value would be too great, and if we do subtract it, the value is less than the true value, therefore the error is less than $1/1100000000000$, and thus is less than $1/1000000000000$.

It is seen how exactly it comes out with such easy calculation involving only additions, subtractions and multiplications, to an extent that is not obtainable with tables. Also if the ratio of the tangent to the radius is anything else, the arc can similarly be found, and this is especially easy when it can be expressed in decimal parts. Again, since now the ratio of the circumference to the radius is given in numbers of any required degree of accuracy, by this also

the ratio of a given arc to the circumference is given, and thus also the quantity of angle for a given tangent will appear with any required degree of accuracy. In this way tables may be corrected, supplemented, or, if need be, enlarged, with no great trouble. Any one who will just remember this fairly easy rule will be able without tables to attain to any required degree of accuracy with very little labor. How great an acquisition this is to geometry, I leave it to those who understand to estimate.

CRITICAL NOTE.

It is difficult to see the object that Leibniz had in writing this long historical prelude to an imperfect proof of his arithmetical quadrature, unless it can be ascribed to a motive of self-praise. This suggestion would seem to be corroborated by the claims that Leibniz makes in the parts where I have quoted his own words in italics in the precis, and by the concluding sentence of the translation given in full. Even if this is so, there may be some plea of justification put forward; for Leibniz appears to have been a man impelled by many contradictory motives, but these I think can all be traced back to one origin. The time in which he lived was a time of great discoveries in geometry; Leibniz knew in his soul that he had it in him to be one of the great men in this branch of learning, but as truly recognized his great disability due to his lateness in starting, and felt that his only chance was to belong to the very exclusive set who corresponded with one another; he saw that the only way of entering this set was to do something brilliant. This may be taken as some excuse for any self-praise that we find, and to a less extent for his, to my mind, undoubted plagiarisms. With regard to the behavior of Leibniz, when charged with these plagiarisms, Sloman is not beyond calling Leibniz a liar point-blank; I prefer to call his statements perversions of the truth, made under stress of circumstances, so that *his reputation as a great and original thinker should not suffer*. For instance, to explain what I mean, I will take the statement of Leibniz to de l'Hospital that he owed nothing to Barrow. As I have said in another place, from one point of view, the point of view that Leibniz would take for the *purposes of this letter*, Barrow would be a hindrance rather than a help to Leibniz, in the formulation of his *algebraical calculus, after he had once absorbed all the fundamental ideas*. That is, it would seem that Leibniz always tries to tell the truth, but to put it in a form that to the uninformed reader will convey quite a wrong impression. Another example of this juggling with words and phrases is given by Sloman, in the shape of a letter from Leibniz, dated August 27, 1676, *and the first draft of the same*; these two read together are very much the same, but read apart convey a totally different impression.

A second characteristic of Leibniz may also be traced back to

his desire to make up for his lateness in starting; that is, the sometimes ridiculous claims that Leibniz makes to discoveries, or rather hints at having made them. An instance is given in the *Historia* (see *Monist*, Oct., 1916, p. 599). "It is required to form the sum of all the ordinates $\sqrt{1-xx}=y$; suppose $y=\pm 1\mp xz$, from which $x=2z/(1+zz)$, and $y=(\pm zz\mp 1)/(zz+1)$; and thus again all that remains to be done is the summation of rationals." Unless we assume that Leibniz never understood in all his life what we now call the change of the variable in integration, which to me seems rather far-fetched, the only reason why this should have been allowed to appear in a tract that was certainly written after 1712, is that Leibniz had never attempted this summation; he had set this down in 1674 and 1675 as a method of quadrature for the circle, not at that time having perceived the importance of the factor dz , or, in other words, the way in which the ordinates should be ordained; for as I have already pointed out, at that time Leibniz could not have found dz , since he could not differentiate a product. This goes to prove that his reading of Pascal was not of the profoundest; for Pascal is very careful over this point, going to the trouble of calling the y 's ordinates when drawn through the points of equal division of the base, and sines when they are drawn through the points of equal division of the arc. Probably to this characteristic is due the claim, set in italics in the manuscript above, with respect to equations of higher degrees. *He thought* he had a general method, which he had not time to verify by particular examples, and so find that his claim was erroneous. For surely this cannot be read as a claim to the Tschirnhausian transformation and the expression of a quintic in the canonical form $x^5+px+q=0$.

The date of the above manuscript is almost certainly antecedent to the manuscript that Leibniz got ready for the press, *De Quadratura*; hence his claim to be able to give examples of the calculus, except for integral powers which had already been done by Wallis, is without foundation.

With regard to the arithmetical quadrature itself, the great importance of it in the estimation of Leibniz is apparently in the correction and enlargement of tables; this claim, as Leibniz puts it, is ridiculous, although it could be so used by first constructing tables for angles whose tangents are given. But Leibniz, after giving a calculation true to twelve places of decimals, states that "the ratio of the circumference to the radius is now known," and proposes to use that. Apparently he does not see that to calculate this ratio from the series he gives, it will be necessary to take a billion or so of terms! For he does not give any hint of any modification of the series, or the use of the value obtained for some small angle.

Lastly, with regard to the calculation, it is strange that the denominator chosen as a common denominator is 15 times what it need have been; also it is a matter of wonder, considering that tables of logarithms were known to Leibniz, as a reader of Mercator and

others, that Leibniz puts the matter in fractional form instead of working in decimals; thus, the arc whose tangent is 0.1 is equal to

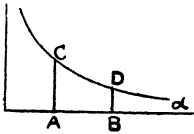
$$\begin{array}{r}
 0.1 \quad \quad \quad - \quad 0.000333333333333333 \text{ ----} \\
 0.000002 \quad \quad \quad \quad \quad 0.00000001428571428 \text{ ----} \\
 0.0000000001111 \text{ ----} \quad \quad 0.000000000000909 \text{ ----} \\
 \hline
 0.100002000111111 \text{ ----} \quad \quad 0.000333347619956 \text{ ----} \\
 \hline
 = 0.99966865249 \text{ ----}
 \end{array}$$

Finally, note that while Wallis and Brouncker are mentioned, Barrow is not. This is all part and parcel of his successful attempt to conceal, from all but Oldenburg, the fact that he had a copy of Barrow in his possession, right from the commencement of his studies.

III.

Transcribed from a manuscript tract of Newton on "Analysis by means of equations with an infinite number of terms."

$AB \sqcap x$, $BD \sqcap y$, a , b , c given quantities, m , n whole numbers. If then



$$ax^{\frac{m}{n}} \sqcap y, \frac{an}{m+n} x^{\frac{m+n}{n}} \sqcap [\sqrt{y}] \sqcap \text{area of ABD.}$$

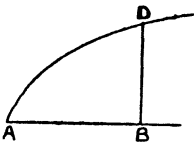
In connection with this the following example is to be noted:

If $\frac{1}{x^2} (\sqcap x^{-2}) \sqcap y$, that is to say, if $a=1$,

$n=-1$, and $m=-2$, then we shall have

$$\left(\frac{1}{-1} x^{-1} \sqcap \right) - x^{-1} \left(\text{or } \frac{-1}{x} \right) \sqcap aBD,$$

produced indefinitely in the direction of a ; the calculation makes this negative because it lies on the other side of BD .



Again, if $\frac{1}{x}$ (or x^{-1}) $\sqcap y$, then $\frac{1}{0} x^0 \sqcap \frac{1}{0} x^0 \sqcap \frac{1}{0} x^1$ (* this

ought to be written $\frac{1}{0} 1^*$) $\sqcap \frac{1}{0} \sqcap$ infinity, which is the area of the hyperbola on either side.

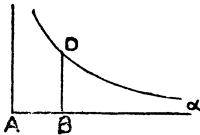
If $\frac{1}{1+x^2} \sqcap y$, on division we obtain

$$y \sqcap 1 - x^2 + x^4 - x^6 + \text{etc.}, \text{ and then}$$

$$ABCD \sqcap \frac{x}{1} - \frac{x^3}{3} + \frac{x^5}{5} - \frac{x^7}{7} + \text{etc.}; \text{ or, if}$$

the term x^2 is the first in the division, the value of y will be $x^{-2} - x^{-4} + x^{-6} - \text{etc.}$,

$$\text{and hence } BD \text{ a } \sqcap -\frac{x^{-1}}{1} + \frac{x^{-3}}{3} - \frac{x^{-5}}{5} + \text{etc.}$$



The first method is to be used when x is small enough, and the second when x is large enough.

Gerhardt then remarks that Leibniz has noted completely the following two cases of extractions of roots:

$$(\sqrt{a^2 + x^2}) \sqcap y, \text{ and } \frac{\sqrt{(1+axx)}}{\sqrt{(1-bxx)}} \sqcap y.$$

Gerhardt further notifies the reader that he has omitted everything that he has found Leibniz to have copied out word for word, on comparison with Biot's edition of the *Commercium Epistolicum* (1856).

In the above, Leibniz marks interpolated remarks of his own with either [] or (* *).

In the same manner, Leibniz has written out word for word the part of the manuscript dealing with the solution of adfected equations (against this he has put the final observation: "And these things that have been given will be sufficient for the investigation of areas of curves"), in addition to the part which follows, "the application of what has been given to other problems of the same kind," which, as being already known to him, he has not copied out. He goes straight on to the next section, "To find the converse of the foregoing, that is, to find the base when given the area, and to find the base when given the length of the curve." He has written this out word for word; also he has noted fully to the end the "proof of the method of solution of adfected equations."

At the end of these extracts from Newton's tract follow the words, "I extracted these things from the letter of Newton 20 Aug. to Newton." Gerhardt states that he has already said all that is necessary about the contents of these extracts.

SECOND SHEET.

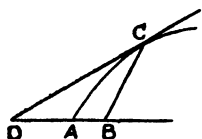
Extracts from the correspondence between Collins and Gregory.

Among a number of partly illegible and unintelligible notes the following were to be noticed.

Gregory, January, 1670: Barrow shows himself to be most subtle in the geometry of optics. I think that he is superior to all whose works I have looked into, and I esteem this author beyond anything that can be imagined.

Sept., 1670: I think that Barrow has gone infinitely further than all those who have written before him. From his method of drawing tangents, combined with certain meditations of my own, I found a general geometrical method of drawing tangents, without calculation, to all curves, which not only contain his particular

methods, but the general method as well. This is shown in 12 propositions.



Letter of Newton, 1672: ABC is any angle, $AB \sqcap x$, $BC \sqcap y$. Take, for example, the equation,

$$\overset{0}{x^3} - 2 \overset{1}{x^2} y + \overset{0}{b} x^2 - \overset{0}{b^2} x - \overset{2}{b} y^2 - \overset{3}{y^3} \sqcap 0.$$

Multiply the equation by an arithmetical progression, both for the second dimension y and for x ; the first product will be the numerator, and the other divided by x will be the denominator of a fraction which will express BD, thus,

$$BD \sqcap \frac{-2 x^2 y + 2 b y^2 - 3 y^3}{3 x^2 - 4 x y + 2 b x - b^2}.$$

Moreover that this is only a corollary or a case of a general method for both mechanical and geometrical lines, whether the curve is referred to a straight line, or to another curve, without the trouble of calculation, and other abstruse problems about curves, etc. This method differs from that of Hudde and also from that of Sluse, in that it is not necessary to eliminate irrationals.

NOTE.

It is almost useless trying to write a critical note on the above in such an incomplete state. But I may remark that Leibniz apparently was at the time quite ignorant of what we now term "putting in the limits for a *definite* integral."

Gerhardt considers that the existence of this extract proves conclusively that Leibniz did not see the letter of Newton so often referred to; forgetting, as Sloman remarks, that Leibniz ought not to have seen the tract at all!

P. S. In allusion to footnotes 3 and 18, with regard to the use of the word "moment" or "momentum" in the sense used by Leibniz, I have found (since the above was written) that Cavalieri, in his *Exercitationes Sex*, defines the term in the mechanical sense and gives much of the matter of Pascal on Centers of Gravity, as it appears in the "Letters of Dettonville." I suggest that Leibniz saw it in Cavalieri, and that its origin is to be traced to Galileo. J. M. C.